some respects, yet differed from it in some important points. The "low sun-bands" appeared weak rather than strong, partly perhaps by contrast with the great intensity of the rainband, and the rainband itself was easily divided into lines, of which eight are recorded in my note-book as being seen with a one-prism spectroscope. The band between *b* and F, observed by Mr. Lockyer, was also seen here, and was found to be one ascribed to aqueous vapour, W.L. 504. A spectrum almost in all respects similar to that observed here can be seen by any one who will examine the absorption produced by a *small* cloud passing over the sun as seen with the spectroscope, having a lens in front of the slit. The contrast with the bright spectrum of the sun shows the general absorption in the red very clearly, and if the sun be near the horizon the other bands will be, in most cases, fairly well seen.

It is worth noting that we have had an unusually early and heavy monsoon, ushered in by a remarkable thunderstorm and followed by a period when the spectrum showed an abnormal freedom from vapour, the rainband at times being quite invisible. During this latter period we have had beautiful rosy after-glows, the sunlight being apparently reflected from thin, almost invisible, cirrus clouds.

If the presence of dust can be proved, these phenomena, as I previously indicated, can be readily explained in accordance with the facts so beautifully illustrated by Mr. John Aitken (*Trans. R.S.E.*, vol. xxx. p. 337), for the dust particles would condense moisture in the upper parts of the air, and we would have a light haze, such as was observed here, not sufficiently dense to cause actual clouds, but deep enough to give the special absorption effects, while the dust itself would assist in producing the general absorption.

Against the idea of Java dust, however, have to be set a number of facts of which the following are a few:—The maximum phase of greenness was on the same day (September 10), all over Ceylon and South India, and as far west as long. 64° (at sea). The green sun was not seen at Rangoon nor at the Andaman Islands, though at the latter place the sounds of the eruption were heard. The first rain that fell here afterwards was subjected to careful microscopic analysis, and showed no trace of volcanic dust. The phenomenon reappeared on September 22.

For my own part I think there is strong evidence that the influence of the Javan eruption was an electrical one, and that that was not necessarily propagated by the actual transference of matter. Mr. Whymper's very interesting letter is of course by no means conclusive as regards the effects of dust, for it is, I believe, regarded as virtually proved that the mere existence of dust in large quantities in volcanic ejecta proves the presence of an abundance of water vapour. C. MICHIE SMITH

P.S.—There is a misprint in my letter to Sir William Thomson which, as I have seen it twice quoted, ought to be corrected. It is in vol. xxix. p. 55, line 8, which should read: "After the electricity had gone to *negative*." C. M. S.

The Christian College, Madras, January 23

SINCE the end of October, when I first observed an unusual red glow for a considerable time after sunset, I have been a close observer of the atmospheric phenomena so fully described by your correspondents. For some time past they have appeared with little of their former brilliancy, until the evening of the 7th inst., when there was a remarkably fine display, equalling in many respects those of December. Of this I shall particularly mention but one feature which I had seen three times previously, but never displayed with such intensity and clearness of definition. At 5.30, when the after glow was at its maximum, a lovely crimson arc appeared opposite it in the eastern horizon, in every respect as described by Mr. Divers in his letter dated from Japan, which appeared in NATURE of January 24 (p. 283). I may remark that I have observed here, from November 10 to this date, but latterly with much diminished intensity, every one of the phenomena he so graphically describes. A. C.

Roscommon, February 11

"The Indians of Guiana"

IN the notice of Mr. Im Thurn's work on the Indians of Guiana, in the current volume of NATURE (p. 305), Mr. Tylor writes: "What is still more curious is that the rude method of making thread by rolling palm or grass fibre into a twist with the palm of the hand on the thigh may be commonly seen in Guiana, although the use of the spindle for spinning cotton is also usual." As such a fact appears to be curious to so eminent an anthropologist as Mr. Tylor, it may be of interest to some of your readers to learn that this mode of twisting fibres is still by no means uncommon in India, though spinning must there have been familiar to the natives for unnumbered generations. I have frequently seen Hindus of various castes twist a mass of jute-fibre into a compact and firm rope of considerable length, between the palm of the hand and the inside of the thigh, and by the same means they will frequently produce long pieces of strongly coherent twine when the need for it arises. From my experience, which, though confined to a small geographical area, comprehended an acquaintance with both Hindus and Mohammedans imported into the tea-districts from almost every part of British India, I should suppose that this custom of twisting fibres into rope and twine is universal throughout the country, though doubtless it is resorted to rather as a makeshift than as a regular mode of manufacturing twisted cords. That such a means should be resorted to by the wild tribes of the north-eastern frontier is by no means strange, though these have acquired not a little skill in spinning and weaving cotton, but that so primitive a method should still prevail amongst peoples so highly cultured as the Hindus and Mohammedans of India often struck me as remarkable.

While noticing Mr. Tylor's interesting article, I cannot refrain from questioning the justice of the supposition that pile-dwellings on the land are due to the "survival of the once purposeful habit of building them in the water." That in New Guinea such is the case there can be little doubt, as Dumont d'Urville and Mr. Wallace, as well as Prof. Moseley, have remarked. And that Mr. Im Thurn's supposition with regard to the natives of Guiana is also correct there can hardly be a doubt. But these two cases scarcely seem to me sufficient upon which to generalise, even when added to Prof. Moseley's pretty and ingenious view as to the origin of the Swiss châlet. As has been pointed out to me by my friend Mr. W. E. Jones, F.R.I.B.A., Lecturer on Architecture in the Bristol University, a somewhat similar development of single-storied into two-storied dwellings is to be traced in the stone buildings as well as in the less substantial dwellings of Western Asia, between the twentieth and the twelfth centuries B.C., and though of course it is not impossible, it certainly seems improbable that a race of ancient lake-dwellers should have perpetuated on sandy plains a practice which must altogether have ceased to be useful long before it reached a region so far removed from its original home. And indeed it seems to me that pile-dwellings may be observed in localities in which it is scarcely possible that the practice could have originated in lake-dwellings, or in any dwellings of any sort erected in water, whether fresh or salt. I allude more particularly to the raised dwellings of the Nagas, Kukis, Cacharis, Khasias, and other hill-tribes of the north-eastern frontier of India, in the midst of which I lived for several years. That these people should ever have dwelt so near the sea that they acquired the habit of erecting pile-dwellings therein seems to me highly improbable when it is remembered that their racial and linguistic affinities place them undoubtedly in that great Mongolian group of which the Thibetans and Burmese are examples; and that therefore they may be regarded as immigrants from more Eastern Asia, rather than as tribes which have been gradually driven back from the Bay of Bengal by the encroaching civilisation of the Hindus. Nor does it seem probable that their pile-dwellings were originally erected in lakes amongst the hills, for in fact the lakes nowhere exist. There are indeed extensive *bheels* or marshes, which during the rainy season sometimes contain a good deal of water. But these *bheels* are, during at least a portion if not the whole of the year, so pregnant with fever and ague that I cannot believe that they were ever employed, as were the lakes of Switzerland and Italy, for the protection of the habitations of man. Yet these north-eastern frontier tribes for the most part build their houses upon piles. These are generally of bamboo, and so of course are very perishable, but occasionally small timber is employed. The floor or platform (of coarse bamboo matting) is seldom raised more than from twenty-four to thirty inches above the ground, though, if my memory serves me, I have occasionally seen it raised as much as between six and seven feet. Beneath this platform a good deal of lumber generally accumulates, and the poultry and pigs frequently congregate for shelter, but I think I never saw an

instance of the lower portion of the erection being inclosed by matting to form a "ground floor." Were these pile-dwellings confined to the low, flat lands upon which the Bengali delights to place his paddy-fields, it would be obvious that they were adopted for the purpose of obtaining a dry, wholesome floor, and security against unanticipated floods. But so far is this from being the case that only very rarely is a Naga or Kuki village to be found on low-lying ground, and generally they are to be seen upon the sides and even the summits of considerable elevations, where any danger from floods is quite out of the question. Again, it might be supposed that these elevated dwellings were adopted as a protection against wild animals but for a curious practice occasionally observable amongst the hill-men. This is the habit of building upon the steep side of a hill in such a manner that the back of the dwelling rests directly upon the ground, while the front is supported upon piles which are of a height sufficient to render the floor, throughout its length, horizontal. Such a plan as this reduces the protection afforded from vermin and wild animals to a minimum, and seems to justify the belief that the fear of these creatures at least could have little or no influence upon the architectural habits of the hill-tribes of this part of India; and I long ago came to the conclusion that here at least the object of the pile-dwellings was simply to attain in the easiest way a floor which should be exempt from the damp exhalations of a tropical soil. JAMES DALLAS

"Probable Nature of the Internal Symmetry of Crystals"

UNDER this head Mr. Barlow has published in NATURE of December 20 and 27, 1883 (pp. 186 and 205) an interesting and ingenious memoir. The subject being very important, but also very difficult and intricate, a discussion of the new theory may perhaps contribute to render our ideas a little more precise.

Whilst Haüy, Frankenheim, Delafosse, Bravais, and others think a crystal built up of mere congruent particles, which may be either the chemical molecules or rather certain aggregates of them, Mr. Barlow considers the arrangement of the different chemical atoms in the interior of a crystallised compound, and illustrates some facts by this manner of viewing them. I purpose in the following submitting some objections which arise against the deductions of the author. These objections are of a geometrical, chemical, and physical nature; let us begin with the geometrical ones.

The first problem of Mr. Barlow is "to inquire what very symmetrical arrangements of points or particles in space are possible." He comes to this result: "It would appear that there are but five." Then he describes these five arrangements. What conditions are to be fulfilled by an arrangement of points in space which is to be "very symmetrical," is nowhere said. According to this indefiniteness of the fundamental notion, the five kinds of very symmetrical arrangement seem to be found rather by divination than by systematic reasoning. Therefore the foundation of the theory appears somewhat arbitrary; and we may suspect that it is incomplete. We are in fact confirmed in this presumption if we consider the results of a geometric research published in my "Entwickelung einer Theorie der Krystallstruktur" (Leipzig: Teubner, 1879). In this book I have specified all possible arrangements of points that are regular and infinite, I have called a system of points *regular* if the points are disposed around every one point of the system in precisely the same manner as around every other. *There are sixty-six such regular systems of points possible.* According to the peculiarity of their symmetry they are subdivided into groups, which correspond strictly to the known crystallographic systems. Many of those arrangements of points have a hemihedric or tetartohedric character; others have the structure of a screw; and amongst the latter I could even suggest one particular system which represents the internal structure of quartz. The latter result was obtained (*loc. cit.* pp. 238–245) by comparing the crystallographical and optical properties of quartz with those of the known combination of thin laminæ of mica arranged in the manner of winding-stairs, described by Prof. Reusch fourteen years ago. All sixty-six systems are in agreement with the principal law of crystallography, the law of rational segments of the axes (Wiedemann, *Annalen der Physik*, 1882, vol. xvi. p. 489). For example, if we have reason to suppose that a certain one of these systems should represent the structure of a given substance crystallising in hexagonal pyramids, then we derive geometrically the same series of possible pyramids which nature actually exhibits.

Four of Mr. Barlow's five kinds of "very symmetrical arrangements" prove to be extremely particular cases of four general systems of mine. The first, second, and third kinds of Mr. Barlow's result from the systems which I have called the "rhombendodecahedric, cubic, and octahedric system with 24-points-aggregates" ("Entwickelung," pp. 165-168), if we suppose the twenty-four points of the so-called "24-punkter" coinciding in one point, and if we identify this point with the centre of a sphere of Mr. Barlow. Mr. Barlow's fourth kind of "very symmetrical arrangements" results as a particular case from my "3-gängiges 6-punkt-schraubensystem" (*loc. cit.*, Fig. 46), if the sides of all hexagons are supposed to touch one another, and the layers to have convenient distances. Mr. Barlow's fifth kind of symmetry, not being regular in the sense defined above, cannot be found amongst my sixty-six systems. Though every point is surrounded by six neighbouring points at equal distances, the latter have not throughout an identical arrangement. Every point of the first, third, fifth, &c., layers is situated at the centre of a perpendicular prism (with regular triangular base) whose angles bear the six neighbouring points of the system, but around every point of the second, fourth, sixth, &c., layers, the six neighbouring points are situated at the angles of two regular triangles, which do not lie parallel over one another as before, one of them being turned round in its plane 60°.

As my sixty-six systems comprise four of Mr. Barlow's kinds of symmetry, it may be expected that they include other arrangements besides, which may also pass as "very symmetrical." For example, in a cubic aggregate of points, the centres of the edges of all cubes determine a very symmetrical arrangement of points, where every point has equal distances from the next eight surrounding points (cf. "Entwickelung," &c., p. 160). From this I believe I have shown that the geometrical foundation of Mr. Barlow's theory is somewhat arbitrary and incomplete.

I now come to the chemical objections, which I will explain by an example. A chemical compound of two kinds of atoms, present in equal number—for example NaCl—could, according to Mr. Barlow, crystallise into the first or second of his five kinds of symmetry, for either of these two kinds allows the regular arrangement of two kinds of particles in equal number. In the first kind of symmetry (for example) spheres are so arranged that they constitute a cubic system of points, in which the centre of each cube bears also a point of the system. By putting atoms of one kind (Na) on the angles, and atoms of the other kind (Cl) on the centres of the cubes, we have built up the structure of a crystal of NaCl. Thus eight atoms of Na stand in exactly identical manner around an atom of Cl (and also eight atoms of Cl around an atom of Na). The atom of Cl seems consequently to be in equally close connection with eight atoms of Na; it has exactly the same relation to these eight atoms. It appears therefore as *octovalent*, certainly not as univalent; for it would be entirely arbitrary to suppose any *two* neighbouring atoms of NaCl in an especially close connection and to take this couple for the chemical molecule of NaCl. By this example we see *that from Mr. Barlow's point of view both the notion of chemical valency and of chemical molecule completely lose their present import for the crystallised state.* This objection, of course, will not destroy the theory of Mr. Barlow, since chemical valency does not yet belong to perfectly clear and fixed notions, and since the idea of the chemical molecule in a crystal is also not evident and clear. The author, however, is at all events obliged to show why these two notions, of such great moment for substances in a gaseous state, should become completely insignificant, as soon as crystallised bodies are in question.

Finally for a physical objection. With respect to the fact that most substances change their volume in congealing, Mr. Barlow admits that the atoms themselves undergo an expansion (positive or negative) in the act of crystallisation. Thus he attributes to the atoms variability of volume, *i.e.* one of those qualities, for the explanation of which the atomic theory has been devised. Well, let it be so, but this hypothesis of atomic expansion is not even found sufficient everywhere, but must be assisted occasionally by auxiliary hypotheses. Thus for explaining the isomorphism of substances which contain atoms of the same kind (*e.g.* $CaCO_3$ and $FeCO_3$) Mr. Barlow supposes that the expansion in the act of crystallising is confined to the common atoms, whilst the different atoms in both substances remain unaltered.

All these objections do not overthrow the author's theory, but they shake it. Perhaps they will induce Mr. Barlow to establish